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Disentangling retirement and savings responses ☆

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ABSTRACT

In January 2006, the Dutch government implemented a pension reform that substantially reduced the public pension wealth of workers born in 1950 or later. At the same time, a tax-facilitated savings plan was introduced that implied a large savings subsidy for all workers, irrespective of birth year. This paper uses linked administrative and survey data to assess the effect of the reform on the savings and retirement expectations and realizations of two virtually identical male cohorts that differ only in treatment status, the treated having been born in 1950 and the controls having been born in 1949. We show that retirement expectations are in line with realizations and that the reform increased the labor supply for the larger part of the workers, namely, those without sufficient means to substantially increase private savings to counter the effect of the reform. These workers have zero substitution rates between private and public wealth. On the other hand, there is a group of mostly high-wage workers who participate in the tax-facilitated savings plan and increase their private savings to fully counter the impact of the drop in public wealth. An unintended side effect of the introduction of the tax-facilitated savings plan is that high-wage earners who are not affected by the drop in pension wealth retire even sooner than initially planned.

1. Introduction

Most industrialized countries have sophisticated pension systems that provide substantial pension benefits after retirement. These benefits have important effects on employees' intertemporal work and savings choices. Due to population aging and declining fertility rates, many of these countries have started to implement major pension reforms aimed at increasing the labor supply of older workers. Whether and to what extent individuals adjust their retirement and savings plans in response to changes in public pension wealth (PPW) is important. Is private wealth a good substitute for the mandatory buildup of public pensions? And how does this interact with labor supply choices?

This paper looks at the effect of a large permanent change in pension wealth on the private savings decisions and retirement expectations and realizations of Dutch public sector workers born in 1949 and 1950. As of January 2006, the pension rights of those born in 1950 (or later) were substantially reduced, while the pension rights of those born in 1949 (or earlier) were unaffected. The 1949 cohort could retire at age 62 years and three months at 70% of their gross wages. For the 1950 cohort, the gross replacement rate dropped to 64% if they wanted to retire at age 62 years and three months, or they had to work an additional 13 months to obtain the 70% rate of their slightly older counterparts.

At the same time as the pension reform, the Dutch government introduced the so-called Life Course Savings Scheme (LCS), or *Levensloopregeling*, a tax-facilitated savings program that permits tax-free savings of up to 12% of annual earnings in a fund that can be used to finance periods of nonemployment, such as a sabbatical or early retirement. The LCS enables all workers (those affected by the pension reform and not) to privately save at lower costs.

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We use linked administrative and survey data over the period 2007–2014 in which we observe the pension rights, individual retirement expectations, decisions to participate in savings programs, and actual retirement choices of public sector workers born in 1949 and 1950. This setup enables us to apply a sharp regression discontinuity design (RDD) to measure the short-run response (one year after the implementation of the reform) and the longer-run response (up to eight years after the reform).

Our paper relates to the literature on the effects of Social Security and pension on the labor supply and earnings of older workers (e.g., Krueger and Pischke, 1992; Borsch-Supan, 2000; Coile and Gruber, 2007; Mastrobuoni, 2009; Blau and Goodstein, 2010; Gelber et al., 2016; Fetter and Lockwood, 2018). These studies show that (changes in) the incentive structure of these programs can have important effects on retirement timing and earnings. There is a somewhat smaller literature on the effect of the financial crisis on the wealth and retirement of older workers. Gustman et al. (2012) find moderate effects on wealth and labor supply. Goda et al. (2011) and Munnel and Rutledge (2013) find that the crisis induced changes in retirement planning, with stronger effects for individuals who experienced larger economic changes (Goda et al., 2011). McFall (2011) shows that the crises extended the working life of those with private pension wealth by 2.5 months.

Our paper is also of direct relevance to the literature on the substitution between PPW and private savings. Reasoning along the lines of the standard life cycle model, PPW will affect the accumulation of other forms of private savings. In a more general life cycle model, retirement is endogenous and the effect of changes in PPW on private savings is ambiguous (Feldstein, 1974; Feldstein and Pellechio, 1979). Moreover, the rate of substitution between private and PPW depends on a range of factors, such as tax incentives, the relative rate of return to private assets, financial literacy, liquidity constraints, and individual information about pensions (Bottazzi et al., 2006; Alessie et al., 2013). Therefore, the degree of substitution between private and public savings in practice has been the subject of many empirically oriented papers.

It has been found that private wealth and PPW are not perfect substitutes (e.g., Feldstein, 1974; Feldstein and Pellechio, 1979; Gale, 1998; Bernheim, 2002; Engelgardt and Kumar, 2011; Chetty et al., 2014; see also the literature reviewed in Alessie et al., 2013). Attanasio and Bruggiavini (2003) and Attanasio and Rohwedder (2003) argue that the measured effect depends on when in the life cycle the individual experiences the reform, but also when the individual is observed by the researcher relative to the reform. Therefore the degree of substitution between private and public pension wealth may vary with age.

Evidence based on analyses that consider both savings and retirement responses to pension reforms is scarce. An exception is the paper by Bottazzi et al. (2006). The authors exploit a series of pension reforms that took place in Italy between 1992 and 1997 to estimate their effects on households' retirement expectations and private wealth accumulation. In their analyses, the authors compare the pre-reform behavior of cohorts of private sector workers, public sector workers, and self-employed individuals with post-reform behavior. Their findings indicate that workers revised their retirement expectations in accordance with the incentives of the reforms, and those who are better informed about future benefits have higher rates of substitution between public and private pension wealth.

As Bottazzi et al. (2006), we look at the effect of a reform on retirement expectations and savings decisions. However, we differ in some important ways. First, we isolate a single and clear reform that led to a sharp discontinuity design; that is, the assignment rule is clear and simple and implies a strong differential treatment of workers born around January 1, 1950. Second, the two cohorts, one born in 1950 and the other in 1949, are homogeneous with respect to observed characteristics and differ only in treatment status. The cohorts are not subject to other policy reforms that could differentially affect them and prior to the reform both cohorts had identical expectations about the age of retirement. Bottazzi et al. (2006) show that the effect of the reform critically depends on how well the individuals are informed about the reform. We provide evidence that the 1950 cohort understood the consequences of reform for their pension wealth. Third, we start observing both cohorts just after the policy reform in 2007 and follow them up to 2014. This implies that we estimate different elasticities. We measure direct, short run effects, but also how the response evolves as the cohort ages. Finally, we consider a cohort of workers nearing retirement, a group of workers for whom changes in PPW require a timely response and reforms can have potentially dramatic effects. Most, if not all, pension reforms in the developed world include cohorts that are affected very late in the game. The results of this study therefore provide important information for policy makers about the effectiveness of reforms for such workers as well as potentially unintended reform effects.

Our analyses show that the treatment group immediately adjusts their retirement expectations by about 10.5 months. The drop in pension wealth is equivalent to 13 months of earlier retirement. The difference in expectations between the treated and untreated cohorts remains stable in later years. Our results thus imply that the average treated worker compensates for the drop in pension rights mainly at the expense of leisure in retirement. However, the savings rate (as measured by participation in the LCS) of the 1950 cohort is more than twice the savings rate of the controls. Participants for the control and treated cohorts, respectively, in the LCS state that they expect to finance about eight months and 13 months of earlier retirement. Actual retirement observed eight years after the reform in 2014 are in strong accordance with previously expressed retirement expectations.

Note, however, that these results apply to individuals who are nearing retirement. Workers in our sample only had a few years to adjust their savings to counter the retirement effects of the reform. It is therefore conceivable that our findings may not hold for younger workers or for reforms that are announced much in advance.

Bearing this in mind, our findings show that individuals are forward looking and that the reform increased the labor supply for the larger part of the workers, especially for those without sufficient means to substantially increase private savings to counter the effect of the reform. These workers, who generally have lower wages and education levels, thus have low substitution rates between private and public wealth. On the other hand, there is a group of mostly high-wage workers who are less affected by the reform. They participate in the tax-facilitated LCS and are therefore able to cushion the impact of the reform by private wealth. An unintended side effect of the introduction of the tax-facilitated savings plan is the decision of high-wage earners not affected by the drop in pension wealth to retire sooner than initially planned.

This paper proceeds as follows. Section 2 describes the Dutch pension system and the 2006 reform of the public sector's pension system. Section 3 describes the data and variables. Section 4 presents the results of our empirical analyses. Finally, Section 5 summarizes and discusses the implications of our findings.

2. The Dutch pension system and the 2006 reform

2.1. The Dutch pension system

The Dutch pension system consists of three pillars. The first pillar is the National Old Age Pension (AOW), which is the flat-rate basic public old age pension provided by the government to all residents of the Netherlands when they reach the statutory retirement age (65 in 2006). The AOW is a pay-as-you-go system in which current payments are financed by income taxes. The benefit is related to the net minimum wage.

The second pillar consists of earning-related sectoral pension plans. These pension schemes are predominantly of the defined benefit type and fully funded. Sector pensions are negotiated between unions and employer organizations at the sector or firm level and are usually set forth in collective agreements. Participation is mandatory for individual workers, ensuring that each worker is covered by the sector pension. The Dutch Pensions and Savings Act dictates that the administration of the sector pension schemes in the second pillar is delegated to pension funds to which both employers and employees must contribute. The sector pension schemes allow workers to retire before the statutory retirement age.

Until 2006, workers could retire before the mandatory retirement age using the so-called pre-pension scheme ("*prepensioen regeling*"). Contributions to these sectoral pre-pension schemes were tax deductible and amounted to 17.47% and 7.47% of gross wages for employers and workers, respectively (Euwals et al., 2006). Typically, contributions to the sector pension schemes were such that, in 2006, a public sector employee who had served 40 years in the public sector could retire at the age of 62 and three months at a gross replacement rate of 70% of average yearly earnings since 2004. This includes an annuity financed from the pre-pension contributions to bridge the three years before the commencement of the first pillar AOW benefit at the statutory retirement age (age 65). Consequently, early retirement was the social norm in the Netherlands. Before 2006, approximately 80% of all workers retired at the age of 62 or younger, and only 6% retired at the age of 65 (Statistics Netherlands, 2009).

The third pillar consists of voluntarily built up savings supplementary to the public and sector pensions. These are offered by private insurance companies and typically yield annuity payments at retirement age. Due to the well-established public and sector pension systems, the third pillar is less well developed in the Netherlands.[1]

2.2. The 2006 reform of the pension system

In the Netherlands, as in other countries, there has been an ongoing debate about the sustainability of the pension system and the need for reform. As a consequence of this discussion, the Dutch government replaced the pre-pension in the second pillar with a new pension scheme called the ABP Flexible Pension Scheme, administered by the public sector's pension fund, *Algemeen Burgelijk Pensioenfonds* (ABP). The introduction of the new pension scheme was announced in the summer of 2005 and became effective on January 1, 2006, for workers born in 1950 or later and for those born before 1950 who had not worked continuously in the public sector since April 1, 1997. In light of the ongoing discussion about the sustainability of the pension system, the announcement of a reform was not entirely unexpected. What was unexpected, however, was the speed at which the reform was implemented, as well as the strong differential treatment of workers born around January 1, 1950, which came as a surprise when it was announced on July 5, 2005.

This new ABP Flexible Pension Scheme involves (i) a change in eligibility ages for early pension benefits, with an increase to 60 years; (ii) stronger incentives to continue working; and (iii) a small increase in pension contribution payments.[2] Workers born before 1950 remained entitled to the old, more generous early retirement scheme if they had worked continuously in the public sector since April 1, 1997. Such workers could thus retire between ages 55 and 70. Retirement at age 62 years and three months yields a pension benefit with a gross replacement rate of 70% of average yearly earnings since 2004 (€31,500 for the median worker). Due to the reform, a typical employee born in 1950 or later with 40 years of tenure obtains a gross replacement rate of 64% when retiring at the age of 62 years and three months (€28,500 for the median worker). To attain a replacement rate of 70%, these workers have to postpone retirement by 13 months.[3]

In Appendix A, we provide details about the calculation of replacement rates in the old and new system. Table A1 in Appendix A presents the replacement rates by age and earnings quartiles. The table shows that, at any age, the replacement rates in the new system are lower than in the old system and that, in the new system, the replacement rates at lower earnings are generally higher than those at higher earnings. Taken together with differentials in age-related mortality rates across earnings groups, this leads to substantial differences in the present discounted value of PPW across earnings groups.

Figs. 1a–1c plot PPW profiles for retirement at different ages for low-earnings groups (Fig. 1a), median-earnings groups (Fig. 1b) and high-earnings groups (Fig. 1c) in the old (black lines) and new (gray lines) pension system. See Appendix A for details of the PPW calculations. The figures show that differences in PPW are substantial and are largest for higher-earnings groups. Specifically, at age 62, PPW differentials are €18,664, €35,209 and €65,209 for the bottom earnings quartile (€37,000), the median earnings (€45,000) and the top earnings quartile (€60,000), respectively. The figures also show that the PPW accrual rates are higher in the new system, implying greater rewards to delayed retirement. The PPW differential therefore decreases substantially at the statutory retirement age, particularly for lower-earnings groups (€6299, €12,510 and €39,781, for the bottom, median, and top quartile earnings, respectively).

At the time of the pension reform in 2006, the Dutch government also introduced the tax-facilitated savings program LCS, similar to individual retirement accounts (IRAs) in the United States. The LCS, open to all workers, irrespective of their year of birth, permits tax-free savings of up to 12% of annual earnings in a fund that can be used to finance periods of nonemployment, such as a sabbatical or early retirement.[4] The savings are collected from monthly gross wages and held in accounts at insurance companies, banks, or the subsidiary companies of pension funds. All workers are allowed to save up to a maximum of 210% of their annual earnings in the fund. Those born in the years 1950 through 1954 and who therefore had less time to save 210% than younger cohorts are allowed to save more than 12% of their annual earnings, as long as the cumulative maximum does not exceed 210% of annual earnings.[5]

Besides the introduction of the ABP Flexible Pension Scheme, no other institutional changes differentially affect the 1949 and 1950 cohorts in 2006. We can therefore apply a sharp regression discon-

[1] In 2007, it constituted only 5% of retirement income (Bovenberg and Gradus, 2015).

[2] The reform abolished the use of the annuity to bridge the gap between early retirement and age 65 for cohorts born after 1949. However, premiums contributed prior to December 31, 2005, remained exempt from taxation and were included in the stock of pension wealth in the new pension scheme. The small premium increases amounted to 0.4%, or €140 annually for the median worker.

[3] This only holds for the first (1950) cohort. Later-born cohorts were subject to larger cuts in PPW.

[4] Employers are obliged to allow their employees to take a leave financed by the LCS.

[5] Note that workers in the 1950 cohort must make substantial savings (16% of their annual earnings for six years) to finance early retirement at age 62 (rather than retirement at age 63 and one month).

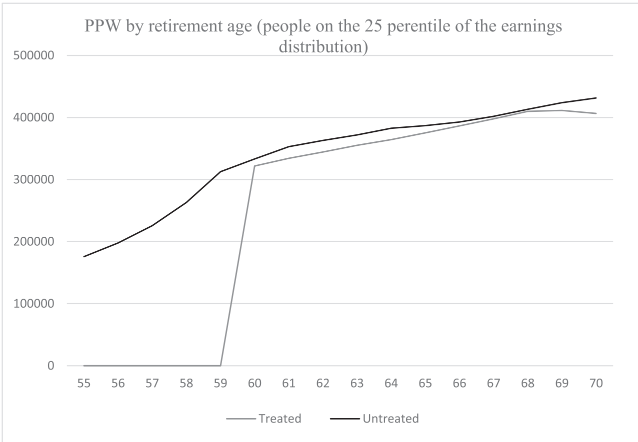

Fig. 1a. Present discounted value of public pension wealth (PPW) by retirement age (for people on the 25 percentile of the earnings distribution). The Present Discounted value of Public Pension Wealth for an individual aged S in 2006 who plans to retire at age R: $PPW_S^R = \sum_{t=R}^{T} B_t^R \cdot a_t \delta^{t-S} - \sum_{t=S}^{R-1} c \cdot Y_t \cdot a_t \cdot \delta^{t-S}$, where a is the conditional survival rate, c the pension contribution rate and δ a 2% discount factor.

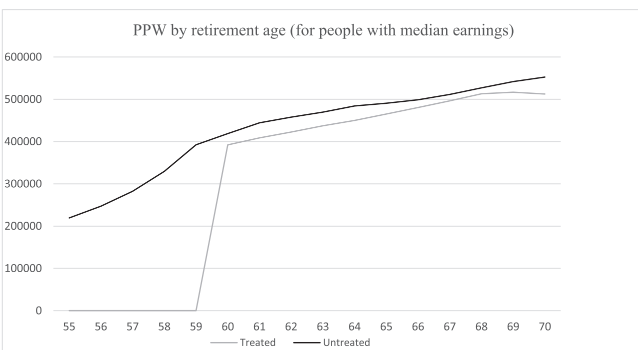

Fig. 1b. Present Discounted Value of Public Pension Wealth (PPW) by retirement age (for people with median earnings). The Present Discounted value of Public Pension Wealth for an individual aged S in 2006 who plans to retire at age R: $PPW_S^R = \sum_{t=R}^{T} B_t^R \cdot a_t \delta^{t-S} - \sum_{t=S}^{R-1} c \cdot Y_t \cdot a_t \cdot \delta^{t-S}$, where a is the conditional survival rate, c the pension contribution rate and δ a 2% discount factor.

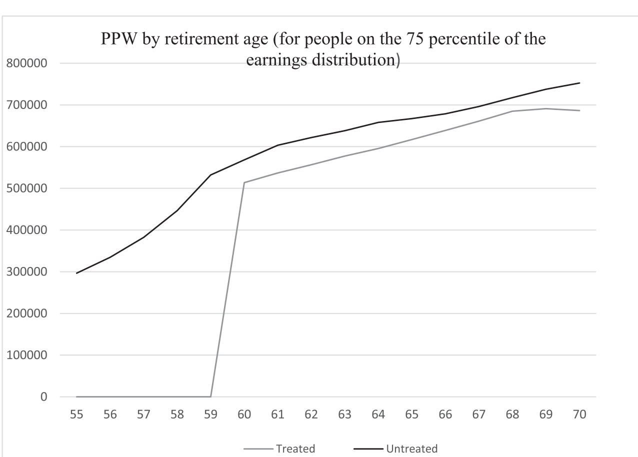

Fig. 1c. Present discounted value of public pension wealth (PPW) by retirement age (for people on the 75 percentile of the earnings distribution). The Present Discounted value of Public Pension Wealth for an individual aged S in 2006 who plans to retire at age R: $PPW_S^R = \sum_{t=R}^{T} B_t^R \cdot a_t \delta^{t-S} - \sum_{t=S}^{R-1} c \cdot Y_t \cdot a_t \cdot \delta^{t-S}$, where a is the conditional survival rate, c the pension contribution rate and δ a 2% discount factor.

tinuity design (RDD). For the internal validity of our research design, it is crucial that the workers born in 1950 be aware of the consequences of the new pension system for their individual situation. After the announcement of the reform in the summer of 2005, the pension fund ABP launched a massive campaign to inform its clients about the new pension and LCS and explain its financial implications. In a special newsletter, unions, employer organizations, and the ABP jointly explained the ABP Flexible Pension Scheme. Furthermore, all 1.2 million ABP members and their employers received a personalized letter about the core characteristics of the new scheme, along with a complete electronic service package. Therefore, one can assume that, on January 1, 2006, most public sector employees born after 1949 and their employers were made aware of the changes in their pension rights. Of course, this must be verified empirically, which we do in Section 3.4.

2.3. Behavioral responses to the policy reform

The reform implied a permanent change in the pension wealth of workers born in 1950. The LCS further implied a subsidy on savings equal to the marginal tax rate. Starting with the behavioral response to the latter, the implied savings subsidy of the LCS for the median worker with an annual salary of €45,000, investing, say, 12% of his salary (€5400) equals €2268. At a higher marginal tax bracket (52% for gross earnings exceeding €54,000), this subsidy could be substantially higher in absolute terms. These tax advantages could induce additional savings and/or could divert savings from other investment accounts to the LCS account. This LCS is therefore expected to unambiguously increase savings in this program for workers who are not financially constrained. Increased private savings can alter the lifetime budget constraint and, therefore, also influence retirement decisions. In this context, this means that the 1949 cohort could use the additional private savings to retire earlier. The 1950 cohort could use increased private savings to (partly) counter the effects of the drop in PPW.

In the extended life cycle model (Feldstein, 1974; Feldstein and Pellechio, 1979), retirement is endogenous and jointly determined with savings decisions. This also applies here. The reform implied a substantive drop in PPW, and PPW accrual rates are higher in the new scheme (see Figs. 1a–1c). Moreover, in the new system, the minimum eligibility age is 60. Therefore, for those affected by the reform who initially (i.e., in the old system) planned to retire before age 60 and who want to stick to that plan, increasing private savings is the only option. For others, the substantive drop in PPW and higher PPW accrual rates incentivize workers to postpone retirement and/or increase private savings. Note, however, that Figs. 1a–1c also show that the gap in PPW between the old and new systems decreases at the statutory retirement age. Therefore, the savings and retirement timing effect could be smaller for workers who, prior to the reform, intended to retire at or after the statutory retirement age.

In sum, the LCS will unambiguously increase savings in this program for workers who are not financially constrained, and possibly total savings. Increased private savings alter individuals' lifetime budget constraint and can therefore also influence retirement decisions. Those born in 1949 could use the additional savings to finance retirement earlier than initially planned, while the 1950 cohort could also use it to (partly) counter the effects of the drop in pension wealth. Regarding the pension reform, the drop in PPW holds for all ages and is accompanied by stronger incentives to postpone retirement. Additional savings are likely to take place via the LCS, since this program comes with a large savings subsidy. It is a challenge to empirically separate the effects of the drop in pension wealth and the introduction of the LCS on savings and retirement decisions. We address this issue in Sections 4.1 (empirical model) and 4.2 (results).

3. Data collection and descriptive analyses

3.1. Data collection

The Dutch pension fund ABP allowed us access to the administrative data of workers born in 1949 and 1950. The administrative data were available from 2006 until 2014 and contain detailed information on individuals' accrued pension rights at ABP, retirement status, pension benefits, annual wage, number of contractual working hours, tenure in the public sector, and employment subsector. For the analyses, we restrict ourselves to men and link them to the survey data.[6] These data sources have also been used in Montizaan et al., 2010, 2015; Montizaan and Vendrik, 2014.

The panel survey started one year after the reform in 2007 and was repeated annually until 2012. For reasons that will become clear later, we only use the data from 2007 to 2011. The data of the first wave were gathered in two stages. In January 2007, we invited all 27,719 male public sector workers born in 1949 or 1950 to participate in our Internet survey and requested their e-mail address. The invitation letter, sent by surface mail, included general information but did not disclose information about the research question or the research strategy, nor did we inform potential participants that the invitation was sent only to public sector employees born in 1949 and 1950. The letter also explicitly assured confidentiality. In the second stage, the 11,458 workers who sent their e-mail address received, in March 2007, an e-mail with a link to the survey. Again, references to the nature of our research question and research strategy were carefully avoided.

In total, 8516 individuals completed the questionnaire in 2007. The response rates of the two birth cohorts were virtually identical, 30.5% and 31.0% for the treated and controls, respectively (see also De Grip et al., 2012, who use the same data). Our analyses are restricted to full-time employees who worked continuously in the public sector since 1997 until 2006, and not in strenuous jobs (e.g., firefighters, police officers).[7] The worker's age is the only criterion that determines whether he is eligible for the (new) restricted or the (old) more generous retirement scheme. After these selections, the final sample in 2007 consists of 6702 men, of whom 3468 were born in 1950 and 3234 in 1949.

The survey was repeated in March of every year. The responses numbered 4142 in 2008, 6048 in 2009, 5600 in 2010, and 4020 in 2011.[8] We extensively check for differential survey participation patterns between the treated and control cohorts. The results of these checks are presented in Tables B1–B4 in Appendix B. These tables show that selection into the survey and subsequent attrition rates do not vary with treatment status, that the characteristics of those lost to attrition in subsequent waves do not differ from those who remained in the survey, and, importantly, that the effect of the reform on retirement expectations in 2007 was not affected by sample attrition (see Table B4 in Appendix B). We will return to the effect of sample attrition on longer-run outcomes in the sensitivity analyses.

3.2. Measuring retirement expectations and private savings behavior

Our main interest lies in investigating how the change in the pension system and the introduction of the LCS affect retirement expectations, saving decisions, and ultimately retirement realizations. To elicit retirement expectations before the reform took place, we asked respondents in 2007 the following survey question: "At what age did you expect to retire five years ago?" To measure the development in retirement preferences and expectations after the reform, we annually asked these two questions: (1) "At what age do you expect to definitively stop working?" and (2) "What would your pension benefit be as a percentage of your net wage if you retire at age 62?"

There is an extensive literature on retirement expectations, and the general consensus seems to be that retirement expectations measured in this way accurately match realizations (Bernheim, 1989, 2009; Dwyer and Hu, 2000; Chan and Stevens, 2004; Benítez-Silva and Dwyer, 2005). In this paper, we observe expectations for five years, as well as retirement realizations until 2014. We can therefore examine, besides behavioral responses to the policy reform, how well expectations match retirement realizations.

Our survey annually includes several questions on pension savings. The survey includes a question on whether the respondent participates in the LCS. Those who do are also asked how many months of earlier retirement they plan to finance out of this LCS. Unfortunately, this question was asked only in the 2009 wave. The survey also asks whether respondents made additional savings arrangements for their pension in the past year and how many alternative sources of (pension) wealth they have. These alternative pension products include (1) pensions built up at pension funds other than ABP, (2) life annuities, (3) life insurance, (4) savings in excess of €15,000, (5) investments, (6) inheritance, and (7) other pension insurance products. Moreover, the survey asks whether the respondent has a partner with his or her own income or pension and whether workers have positive net housing wealth (the value of the house minus the mortgage).

3.3. Descriptive analyses: Comparing the treated and control cohorts

Since our empirical analyses exploit the sharp discontinuity in pension treatment induced by the natural experiment, it is of crucial importance for the internal validity of our design that (1) the individuals in the treatment and control groups are sufficiently similar, (2) the reform was well understood and actually created a sharp discontinuity in expectations and savings, and (3) that the observed characteristics did not discontinuously change across the threshold.

First, as already mentioned, although attrition was substantial, the survey participation rates of both cohorts are very similar for each year of the panel survey, and there is no (differential) drop in the number of observations near the threshold. Second, Table 1 shows descriptive statistics for the treatment and control groups for the 2007 wave. While the first two columns show the respective means, the last column gives the p-value of the coefficient of an RDD regression for a specific covariate, while correcting for the birth date (in days). Fig. C2 in Appendix C plots the average of the covariates for the treated and control cohorts around the cutoff. The table and figure both confirm that the cohorts are similar in characteristics and that the covariates evolve smoothly across the cutoff. We also perform this test for the subsequent years, 2008–2011, and find no significant differences in observables between the two cohorts (results available upon request).

Concerning personal characteristics, we observe that approximately 67% of public sector workers are highly educated, 90% are married, and they are, on average, in good health. The overrepresentation of highly educated workers in the public sector is confirmed in other (Dutch) data sets. Most respondents are employed in the government (47%) and education (41%) sectors and work full-time.

Among the set of wealth variables, one is significantly different at conventional levels between the control and treatment groups:

[6] We focus on male employees because, in the Netherlands, only a small, selective fraction of women are still working in this birth cohort, at age 57 or 58.

[7] Firefighters and ambulance and police personnel have other retirement schemes.

[8] The increase in the number of responses in 2009 is due to a renewed invitation to participate in the Internet survey. This invitation was sent to counter the substantive panel attrition in the second wave.

Table 1
Characteristics for affected and unaffected respondents (2007).

	Affected by the reform	Not affected by the reform	Min	Max	*p*-Value[1]
Personal characteristics					
Low education level	0.132	0.125	0	1	0.859
High education level	0.671	0.681	0	1	0.518
Married	0.904	0.919	0	1	0.683
Number of sick days	9.47	8.89	0	250	0.741
Job characteristics					
# years contributed to pension fund	30.262	31.762	10	45.579	0.746
Log wage	10.788	10.801	9.69	12.89	0.563
Number of contractual work hours	0.996	0.996	0.26	1.25	0.977
Sectors					
Government	0.481	0.452	0	1	0.763
Education	0.446	0.479	0	1	0.612
Privatized	0.073	0.069	0	1	0.689
Income and savings					
Life course savings (LCS)	0.155	0.064	0	1	0.000
Extra pension savings in previous year	0.254	0.210	0	1	0.197
Partner with own income	0.749	0.728	0	1	0.150
Partner with own pension	0.571	0.572	0	1	0.820
Positive net housing wealth	0.687	0.684	0	1	0.267
Number of alternative wealth sources	2.365	2.359	0	9	0.719
Retirement expectations					
Expected retirement benefit	66.725	72.272	30	135	0.000
Expected retirement age	63.472	62.734	57	70	0.000
Expected retirement age before reform	61.390	61.489	53	70	0.630

[1] The last column reports the p-values for the treatment dummy from an RD regression of the variable in question.

the response to the question whether individuals participated in the tax-facilitated LCS. Only 6.4% of the 1949 cohort participated in this LCS, whereas this fraction for the 1950 cohort is more than twice as high (about 15.5%). Because this program was introduced at the same time as the pension reform, the participation rates in the LCS for the treated cohort could be seen as a direct response to the savings subsidy plus the drop in PPW. For the controls, participation in LCS reflects the direct response to the savings subsidy. Note that the savings subsidy could also indirectly affect retirement choices (see Section 2.3).

3.4. The respondents understood the consequences of the reform

As mentioned at the end of Section 2, the consequences of the reform were communicated extensively to the affected cohorts. Moreover, all employees receive annually a detailed overview of their pension rights that shows them exactly the pension benefits they would receive if they were to retire at different ages. The sample means in Table 1 show that respondents born in 1949 expected a net replacement rate of 72% if they retired at age 62, compared to 65% for respondents born in 1950. Fig. 2 depicts this graphically, with each dot representing the average expected pension benefit for individuals born in a specific birth month. The figure confirms that the respondents understood the consequences of the reform and that there is a strong discontinuity at the threshold date. We find a similar pattern for later years (see Fig. C1 of Appendix C).

The sample means of the question on the expected retirement age five years ago (measured in 2007) do not differ between the treated and control cohorts. Fig. 3 also shows the lack of a clear break around the threshold. This is confirmed in Table C1 of Appendix C.[9] Things look very different for the expected retirement age one year after the reform: those born in 1949 expected, on average, to retire at age 62 years and eight months, while those born in 1950 expected to retire at age 63 years and six months, and there is a clear discontinuity at the cutoff (see Fig. 4). Expectations can change over time. We examine longer-run changes in Section 4.3.

[9] A Kolmogorov–Smirnov test of the equality of the distribution functions indicates no significant differences between the cohorts (results available upon request).

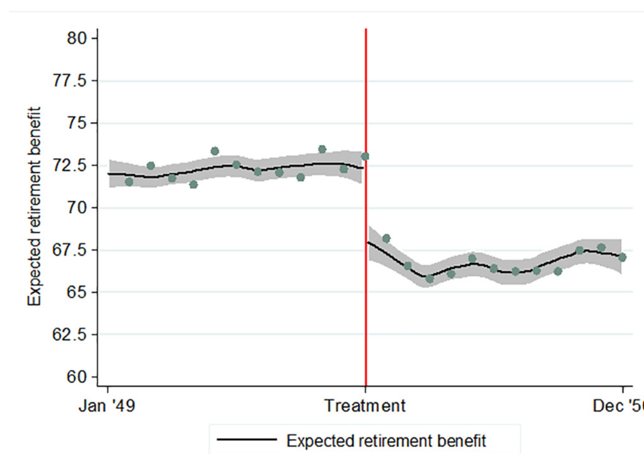

Fig. 2. Expected retirement benefit in 2007. This Figure presents the mean expected pension benefit at age 62 as a percentage of The figure presents the (Epanechnikov) kernel-weighted local polynomial plots of the expected retirement benefit (for two successive birth months). The vertical line marks the threshold dividing the control and treatment groups. The confidence intervals are at the 95% level.

4. Empirical implementation and results

4.1. Empirical implementation

The LCS affected both the 1949 and 1950 cohorts and, reasoning along the lines of the extended life cycle model (Feldstein, 1974), we argue that this savings program unambiguously increased private savings in the program and could lead workers to divert savings from other accounts to LCS accounts. Additional private savings alter the lifetime budget constraint and could lead to earlier retirement. The 2006 pension reform affected only the 1950

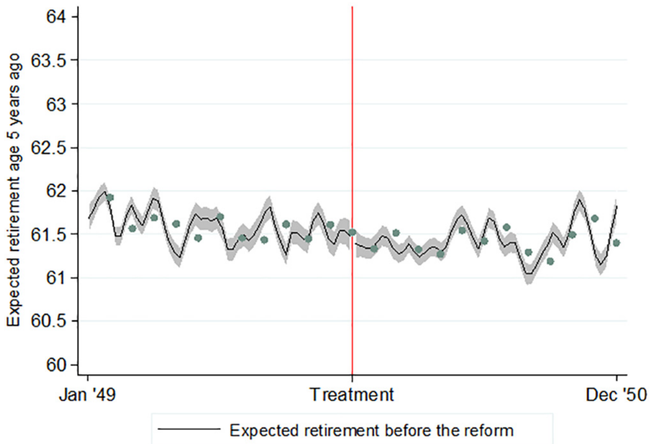

Fig. 3. Expected retirement age before the reform. This figure presents the (Epanechnikov) kernel-weighted local polynomial plots of the expected retirement age in 2002 (retrospectively measured in 2007). The vertical line marks the threshold dividing the control and treatment groups. The confidence intervals are at the 95% level.

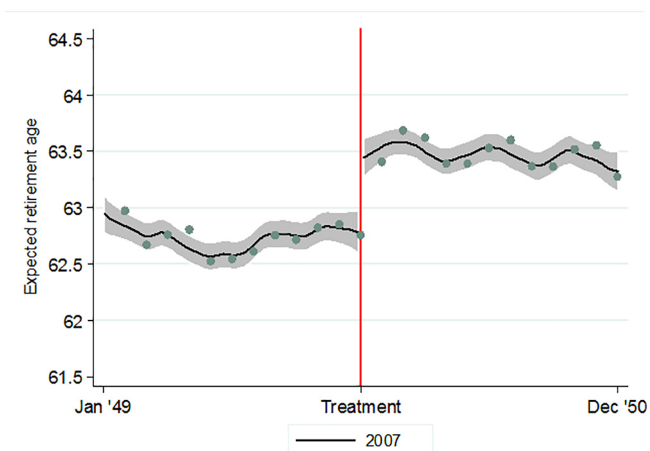

Fig. 4. Expected retirement age one year after the reform (2007). The figure presents the (Epanechnikov) kernel-weighted local polynomial plots of the expected retirement age in 2007 (for two successive birth months). The vertical line marks the threshold dividing the control and treatment groups. The confidence intervals are at the 95% level.

cohort. It implies a substantive drop in PPW, an increase in the minimum eligibility age by five years, and changes in PPW accrual rates (cf. Figs. 1a–1c). This could induce workers born in 1950 to delay retirement and increase private savings.

Given the differential treatment of the two cohorts on January 1, 2006, it is most natural to specify quasi-reduced form Regression Discontinuity (RDD) equations for retirement expectations and savings. More specifically, for changes in retirement expectations $ER_{it} - ER_{i2002}$, we specify

$$ER_{it} - ER_{i2002} = \alpha_0 + \alpha_1 I_i(1950) + \alpha_2 I_i(1950)B_i + \alpha_3 B_i + \alpha_4 X_{it} + \epsilon_{it} \quad (1)$$

Similarly, for savings S, we write

$$S_{it} = \beta_0 + \beta_1 I_i(1950) + \beta_2 I_i(1950)B_i + \beta_3 B_i + \beta_4 X_{it} + \epsilon_{it} \quad (2)$$

where ER_{it} is the expected retirement age in year t, t = 2007, ..., 2011. For S, we distinguish whether an individual participated in the LCS and whether they made (other) additional savings in the past years to supplement their pension.[10] The variable $I(1950)$ is an indicator for the 1950 cohort, B denotes the running variable birth date (expressed in days, normalized to zero on December 31, 1949, and divided by 100), and X is a set of observed individual characteristics, some of which are from the survey and some from the administrative database.

The coefficient α_1 measures the reduced-form treatment effect on retirement expectations. This treatment effect is the compound effect of the direct effect of the change in pension wealth on the labor supply and the indirect savings effect. In turn, this indirect savings effect consists of an effect caused by the drop in pension wealth plus an effect caused by the subsidy on savings due to the introduction of the LCS. Note that the LCS also affected the control cohort born in 1949. The large savings subsidy could affect their private savings decisions and, therefore, also the budget constraint. This, in turn, could also affect the retirement decisions of the 1949 cohort. Consequently, the parameter α_1 measures the changes in retirement expectations of the treated, over and above the changes in the retirement expectations of the controls. Similarly, the parameter β_1 measures the additional effect for the treated (1950) cohort and depends on the savings response to the drop in PPW and the savings subsidy.

The variables S and dER are jointly determined and, therefore, besides our (single) instrument, more information is required to tease out the direct and indirect effects contained in α_1 and β_1. Fortunately, the 2009 survey also contains a question about the number of months the participants in the LCS expected to finance from this program. With this question we can separate savings effects induced by the drop in pension wealth and the effect of the savings subsidy. This, however, requires additional assumptions that we make explicit in Section 4.2.2.

4.2. Short-run effects of the reform

4.2.1. Retirement expectations one year after the reform

The first two columns of Table 2 provide the ordinary least squares (OLS) results of Eq. (1) for 2007. The treatment dummy refers to the average treatment effect of the drop in PPW and the introduction of the LCS for the cohort born in 1950. From a comparison between Columns (1) and (2), one can conclude that adding controls does not alter the parameter estimate of interest, confirming that the treatment and control groups are very similar. Table 1 shows that, in 2002, the retirement expectations were virtually identical for the two cohorts. We therefore also include the results of a regression with the left-hand side of (1) replaced by retirement expectations in 2007 (Columns (3) and (4)). As expected, the treatment effect is very similar in both specifications.

The coefficient of interest (α_1) indicates that, in the short run, workers affected by the reform expect to work 0.88 years (about 10.5 months) longer. Since the drop in PPW is equivalent to 13 months of earlier retirement, our results imply that the average treated worker compensates for the drop in PPW mainly at the expense of leisure in retirement. Note once more that this total average treatment effect on the treated consists of two forces: the adjustment in retirement years (direct effect) and a savings effect (indirect effect).

The regression results in Column (2) of Table 2 also show that the change in retirement expectations is smaller for the higher educated and those with higher earnings.

4.2.2. Savings one year after the reform

Table 3 provides the results for two of our savings measures that we expect to be responsive to the reform: whether individuals

[10] We only observe whether an individual has saved in various ways, and not the actual amount of savings (see Section 3).

Table 2
Expected age of retirement in 2007 and the difference between the expected ages of retirement in 2007 and 2002: OLS results.

	(1)	(2)	(3)	(4)
Dependent variable:	ER(2007)–ER(2002) Change in retirement expectations		ER(2007) Expected retirement age 2007	
Treatment dummy	0.841***	0.875***	0.812***	0.810***
	(0.102)	(0.102)	(0.081)	(0.080)
Birth date/100	0.560*	0.543	−0.053	−0.073
	(0.339)	(0.339)	(0.262)	(0.258)
Birth date/100 * Treatment dummy	−1.188**	−1.166**	−0.300	−0.390
	(0.480)	(0.479)	(0.387)	(0.383)
Married		0.264***		0.056
		(0.097)		(0.079)
Low educated		−0.016		−0.230***
		(0.102)		(0.077)
High educated		−0.204**		0.064
		(0.086)		(0.068)
Wage (ln)		−0.387***		0.124
		(0.124)		(0.103)
Number of contribution years to the pension fund		0.028***		−0.052***
		(0.005)		(0.004)
Constant	1.358***	4.685***	62.724***	63.031***
	(0.073)	(1.323)	(0.056)	(1.100)
Observations	6605	6476	6702	6569
Adjusted R^2	0.041	0.062	0.046	0.093

Robust standard errors are in parentheses; other control variables include sector dummies, in Columns (2) and (4).
* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$.

Table 3
Savings in 2007: Results from a linear probability model.

	(1)	(2)	(3)	(4)
Dependent variable:	Life course savings (LCS)		Extra pension savings in previous year	
Treatment dummy	0.082***	0.083***	0.027	0.028
	(0.017)	(0.017)	(0.021)	(0.021)
Birth date/100	0.017	0.016	0.062	0.046
	(0.047)	(0.047)	(0.070)	(0.070)
Birth date/100 * Treated dummy	0.019	0.009	−0.029	−0.038
	(0.081)	(0.080)	(0.099)	(0.098)
Married		0.016		0.022
		(0.014)		(0.018)
Low educated		−0.004		−0.061***
		(0.013)		(0.017)
High educated		0.018		0.019
		(0.014)		(0.017)
Wage (ln)		0.090***		0.010
		(0.022)		(0.026)
Number of contribution years to the pension fund		−0.003***		−0.010***
		(0.001)		(0.001)
Constant	0.067***	−0.842***	0.222***	0.397
	(0.010)	(0.229)	(0.015)	(0.271)
Observations	5245	5244	6645	6633
Adjusted R^2	0.020	0.039	0.002	0.026

Robust standard errors are in parentheses; other control variables include sector dummies, in Columns (2) and (4).
*$p < 0.10$.
**$p < 0.05$.
*** $p < 0.01$.

participated in the tax-facilitated LCS (Columns (1) and (2)) and whether they had extra pension savings in the previous year (Columns (3) and (4)). Fig. 5 gives a graphical representation. Columns (1) and (2) indicate that the probability of participating in the LCS is about eight percentage points higher for the treated cohort. This effect is virtually unchanged when we add other regressors. Of these other regressors, wages and the number of years an individual contributed to the public pension fund are the only significant variables. Of the two variables the wage seems to be the most important one. A one standard deviation change in the logarithm of wages is associated with a 2.5 percentage point change in the probability to participate in the LCS.[11] The wage effect could reflect the ability to save. Of course, we cannot rule out that the plan is less attractive for low-wage workers, because its tax advantage is

[11] Including wage in the regression increases the adjusted R-square with 9%. Including the number of years an individual contributed to the public pension fund increases the adjusted R-square with 2%.

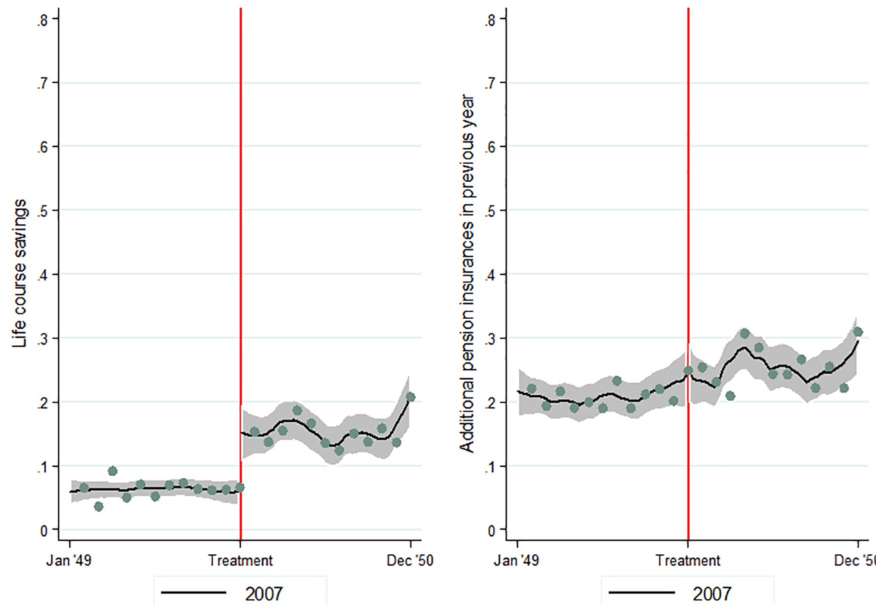

Fig. 5. Additional pension wealth (2007). The figures present the (Epanechnikov) kernel-weighted local polynomial plots of LCS participation and whether workers invested in additional pension insurance in the previous year (for two successive birth months). The vertical line marks the threshold dividing the control and treatment groups. The confidence intervals are at the 95% level.

particularly advantageous for those facing high marginal tax rates, that is, high-wage earners.

Columns (3) and (4) of Table 3 show no significant treatment effects on extra pension savings.[12] This suggests that, for the treated, savings in the LCS are additional savings that are not at the expense of savings in other accounts. About 6.4% of the 1949 cohort participated in the LCS. For this cohort we also run a regression of extra pension savings on participation in the LCS, to see if savings in other pension savings account are crowded out by savings in the LCS. We do not find a significant association. This result suggests that savings in the LCS are additional private savings for the 1949 cohort as well. This finding contrasts with the results for IRAs in the United States of Gale and Scholtz (1994), who find that changes in the contribution limits of IRAs have a small or no net effect on total savings.

A further look at the other coefficients in Column (4) reveals that the effect of the number of years of contribution to the pension system has a sizable effect. This coefficient is more than three times higher than in Column (2), which could suggest that the number of years of contribution to the pension fund is likely to reflect the need to save and that these savings already existed prior to the reform.

In sum, the drop in pension wealth and the savings subsidy increased total private savings, and this occurred primarily via the LCS. Wages have a strong independent effect (irrespective of treatment status) on the decision to participate in the LCS. The number of years of contribution to the pension fund has independent effects on both participation in the LCS and additional savings to supplement pension income.

Recall that the pension reform and the LCS were introduced at the same time, on January 1, 2006. The 1950 cohort was confronted with both changes, whereas the controls were only affected by the introduction of the LCS. Changes in the private savings of the controls are therefore a response to the savings subsidy,[13] whereas changes observed in the savings of the treated are, in addition, affected by the loss in PPW.

For the LCS participants, the 2009 survey also included a question about the number of months they planned to finance from this savings account. Fig. 6 presents this graphically. The figure indicates that those born in 1949 planned to finance an earlier retirement by about eight months from their savings account. The average for the affected (1950) cohort is about five months more. This result is confirmed in Table 4. These five months may be interpreted as a pure substitution effect between private and public pension wealth, that is, a pure crowding out effect of 38% due to the drop in PPW. This, however, requires some strong assumptions. First, that the controls serve as a good counterfactual for the behavior of the treated.[14,15] Second, that the effects of the LCS on retirements and savings are additive with the effect of the reduction in pension wealth. These assumptions have consequences for the preference parameters in a life cycle model. More specifically, for the two assumptions to hold we require intertemporal separable lifetime utility; additive within period utility functions; a constant elasticity of intertemporal substitution and a constant discount factor. We refer to Appendix E for more details. The figure also suggests that treated workers participating in the LCS plan to fully counter the effect of the reform on retirement (i.e. to counter the 13 months of later retirement).[16]

Participation rates in the savings scheme differ for the treated and control cohorts, and while the participating treated and con-

[12] We also looked at other forms of savings and find no treatment effects (results available upon request).

[13] There exists no control group for the introduction of the savings plan, so part of the effect for the controls could be due to secular changes (e.g., expectations regarding future reforms).

[14] We examined whether the characteristics of LCS participants and nonparticipants are similar. Workers in the utility sector are slightly overrepresented in the control group.

[15] Treated workers who were confronted with a drop in their pension wealth were forced to rethink their retirement options and may have informed themselves better. We used information on actual benefits from the pension fund and find that the treated were slightly more accurate in predicting their actual pension benefit.

[16] Wishful thinking may play a role here. If this is the case, then such optimistic views of the treated who decide to participate in the LCS program are included in the effect estimate in Table 4.

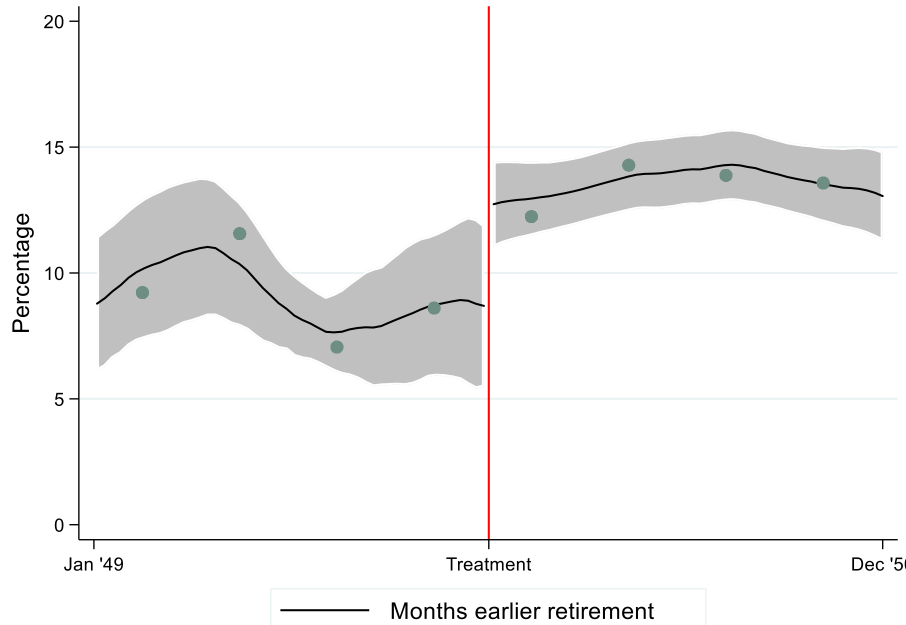

Fig. 6. Months of earlier retirement due to the Life course savings (LCS) (2009). The figure presents the (Epanechnikov) kernel-weighted local polynomial plots of the number of months that workers expect to save in the LCS (for two successive birth months). The vertical line marks the threshold dividing the control and treatment groups. The confidence intervals are at the 95% level.

Table 4
Months of early retirement financed from the LCS in 2009: OLS results.

	(1)	(2)
Dependent variable:	Months of early retirement financed from the life course savings program	
	Column 1	Column 2
Treatment dummy	4.796***	5.008***
	(1.790)	(1.879)
Birth date/100	−4.857	−5.396
	(7.533)	(7.741)
Birth date/100 * Treatment dummy	7.826	6.958
	(8.547)	(8.841)
Married		0.876
		(1.598)
Low educated		−1.084
		(1.716)
High educated		1.922
		(1.206)
Wage (ln)		−1.331
		(1.529)
Number of contribution years to the pension fund		−0.092
		(0.081)
Constant	8.138***	23.567
	(1.575)	(17.168)
Observations	601	589
Adjusted R^2	0.032	0.047

The number of observations is lower, because in this table we focus only on workers who participated in the LCS. Robust standard errors are in parentheses; other control variables include sector dummies, in Column (2).
* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$.

trol groups are very similar in terms of most observables, the differential participation in the savings program could affect the RDD estimate. We therefore also perform OLS and Tobit regressions that include the zeros. OLS regressions based on the full sample yield a coefficient of 2.02 (s.e. = 0.383). Tobit regressions for the LCS participants yield a marginal effect of 2.74 months (s.e. = 0.368). The results are available upon request.[17]

The results of Table 2 indicate that, on average, the treated work about 10.5 months longer (0.875 * 12 = 10.5 months). Of the treated sample, about 16% of those who participated in the LCS intended not to work 13 months longer (see Fig. 5) but, rather, to stick to the previously set retirement age. This implies that the treated who did not participate in the program, on average, expected to work 10.5/0.84 = 12.5 months longer, that is, almost the entire effect of the reform. Hence, it appears that these workers are not willing or able to sacrifice future pension income or current consumption to counter the drop in PPW.

The results of Table 3 indicate that wages are an important factor in explaining the decision to participate in the LCS. In light of the above, this finding could imply that primarily lower-wage workers are induced to postpone retirement. However, these workers generally also have worse health.[18] Indeed, a regression of the logarithm of wages on sickness absence and a set of other controls shows a significant negative association between wages and sickness absence (results available upon request). This finding suggests that financial constraints, rather than a low disutility of work, are at play in the decision to postpone retirement.

4.3. Longer-run effects

4.3.1. Developments in retirement expectations (2008–2011)

The above results show strong initial responses to the reform on retirement. Of interest is whether retirement expectations change in the longer run and, ultimately, whether expectations match realizations. Starting with expectations in the longer run, we estimate model (1) for 2008 to 2011. It is important to note that our longer-run analyses focus on respondents who responded at the

[17] The marginal effect of participating in the LCS, based on the Tobit regression, equals 0.111 (s.e. = 0.015).
[18] See also the literature on the association between socioeconomic status and health (e.g., Banks et al., 2006).

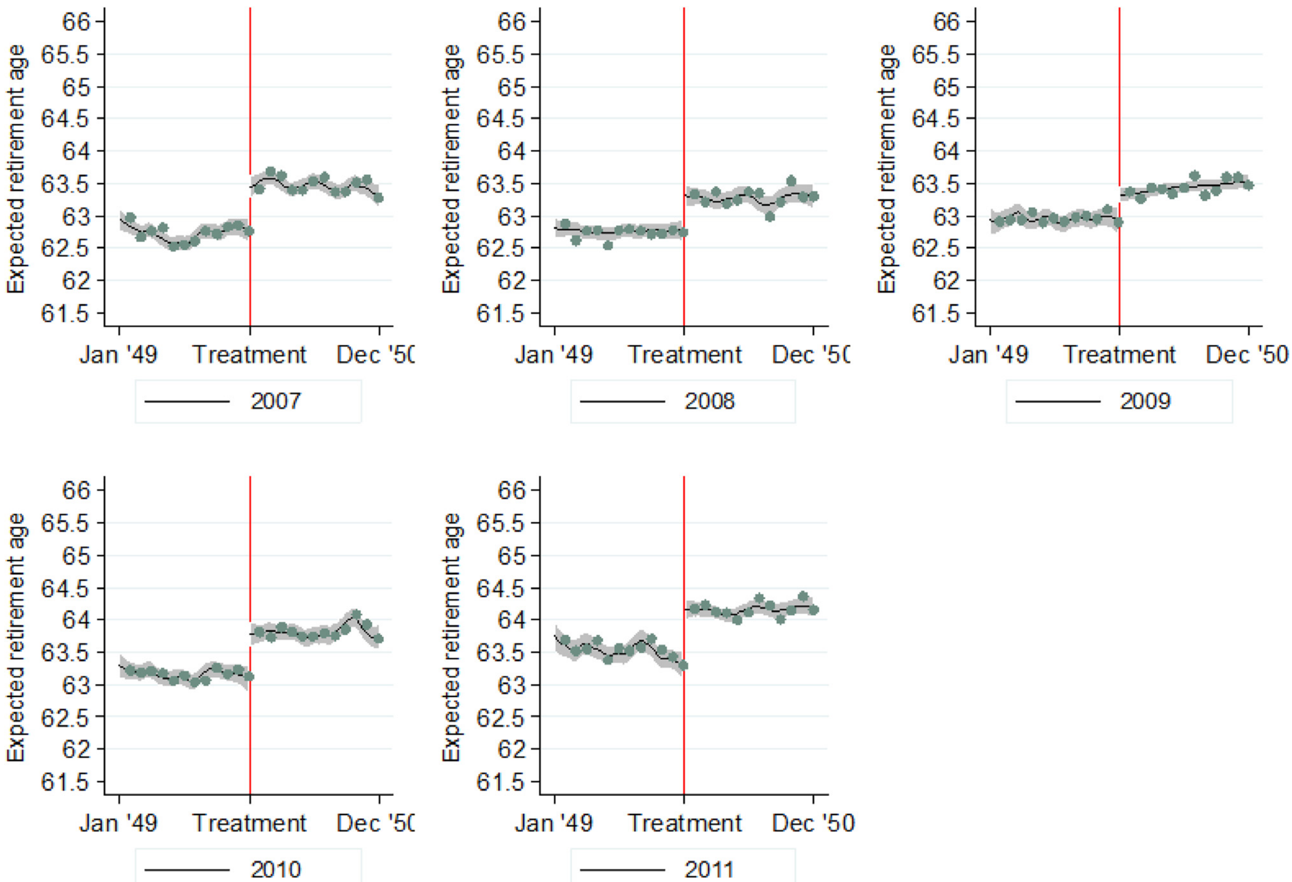

Fig. 7. Developments in expected age of retirement. The figures present the (Epanechnikov) kernel-weighted local polynomial plots of the expected retirement age for the period 2007–2011 (for two successive birth months). The vertical line marks the threshold dividing the control and treatment groups. The confidence intervals are at the 95% level.

start of the panel, in 2007, since we wish to follow the expectations of the respondents relative to their response about their expectations in 2002. We therefore do not use the additional response from the renewed invitation to participate in the Internet survey in later years.

In March 2011 (when the survey was held), 13.8% of the 1949 cohort and 3.7% of the 1950 cohort had retired. After 2011, these fractions rose rapidly (notably for the 1949 cohort; we return to examine actual retirement patterns in the next section). We therefore decided not to use the 2012 wave and to restrict ourselves to retirement expectations in 2008–2011.

Below, we discuss the results for the change in the expected retirement age, $dERit$, t = 2007, …, 2011. Table C2 in Appendix C and Fig. 7 reports the result for the retirement expectation levels, that is, $ERit$, t = 2007, …, 2011. The results of these regressions are very similar to the results discussed below. This also indicates that the conditioning on presence in the initial sample (required for $dERit$ regressions) does not influence our results.

The first column of Table 5 repeats the results of Column (1) of Table 2, while the other columns show the results for later years. The most important result is that the longer-run treatment effects are not much different from the short-run effect. Therefore, it seems that the readjustments took place shortly after the reform and the workers stuck to these in later years. The consistency in treatment effects can most likely be explained by the fact that we are analyzing the behavior of older workers confronted with substantial changes in pension wealth only a few years prior to their initially planned retirement. The situation could be different for younger cohorts.

We finally also ran regressions for participation in the LCS for later years. The results of these regressions are presented in Table C4 of Appendix C and show that the treatment effects on this outcome variable remain relatively constant over the later years. While this result is interesting in itself, it also attests to the stability of the findings on retirement expectations in Table 5.

4.3.2. Retirement realizations

The survey data are linked to administrative data containing information on actual retirement up to March 2014, which implies that those born in the first months of 1949 have turned 65, the statutory retirement age. Therefore, besides differences in pension rights and eligibility conditions, the pure age effect will also lead to substantially higher retirement ages for the controls. Fig. 8 confirms this effect. The retirement rates of the 1949 cohort range from about 70% to more than 90% for those born in the first quarter of the year. In contrast, the retirement rates of the 1950 cohort are 40–50 percentage points lower. The regression results in Table 6 show, controlling for age, that the pure treatment effect is about 26 percentage points.[19]

The treated group does not reach the statutory retirement age by the end of the sample period, while part of the control group does. We therefore run a regression excluding individuals born in the first quarter of the year. This change hardly affects the estimate (−0.276, s.e. = 0.034).[20] Fig. 9 plots the cumulative retirement age

[19] The results based on the administrative data (24,381 observations) show an effect of 0.238 (s.e. = 0.012).

[20] A model for actual retirement in 2013 yields, as expected, a substantially smaller effect (−0.171, s.e. = 0.023).

Table 5
Longer-run effects in retirement expectations.

Dependent variable:	(1)	(2)	(3)	(4)	(5)
ER(t)–ER(2002)	2007	2008	2009	2010	2011
Treatment dummy	0.875***	0.803***	0.602***	0.652***	0.750***
	(0.102)	(0.139)	(0.146)	(0.157)	(0.177)
Birth date/100	0.543	0.066	0.421	1.098**	0.705
	(0.339)	(0.458)	(0.467)	(0.509)	(0.609)
Birth date/100 * 8Treatment dummy	−1.166**	−0.524	−0.467	−1.205	−0.575
	(0.479)	(0.651)	(0.693)	(0.746)	(0.864)
Married	0.264***	0.443***	0.355**	0.523***	0.488***
	(0.097)	(0.142)	(0.147)	(0.169)	(0.183)
Low educated	−0.016	0.123	0.007	0.135	0.077
	(0.102)	(0.140)	(0.157)	(0.176)	(0.245)
High educated	−0.204**	−0.299***	−0.324**	−0.363***	−0.205
	(0.086)	(0.114)	(0.132)	(0.139)	(0.155)
Wage (ln)	−0.387***	−0.242	−0.224	−0.166	−0.385*
	(0.124)	(0.168)	(0.186)	(0.195)	(0.224)
Number of contribution years to the pension fund	0.028***	0.026***	0.032***	0.023**	0.011
	(0.005)	(0.007)	(0.008)	(0.009)	(0.010)
Constant	4.685***	3.007*	2.962	2.707	5.682**
	(1.323)	(1.800)	(1.987)	(2.092)	(2.445)
Observations	6476	3393	3183	2615	2101
Adjusted R^2	0.062	0.058	0.051	0.074	0.072

The regressions also control for subsectors. Robust standard errors are in parentheses.
* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$.

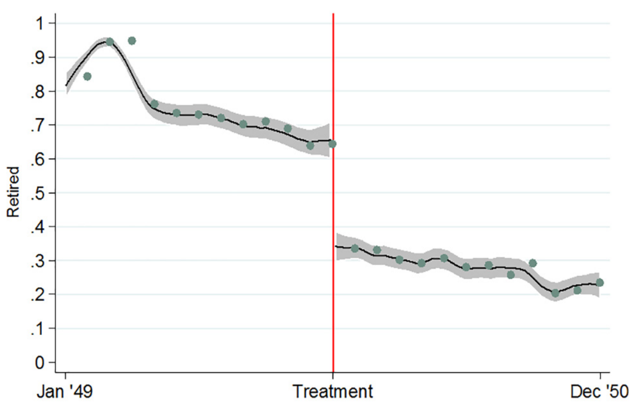

Fig. 8. Retirement realizations in 2014. The figure presents the (Epanechnikov) kernel-weighted local polynomial plots of the retirement rates in 2014 (for two successive birth months). The vertical line marks the threshold dividing the control and treatment groups. The confidence intervals are at the 95% level.

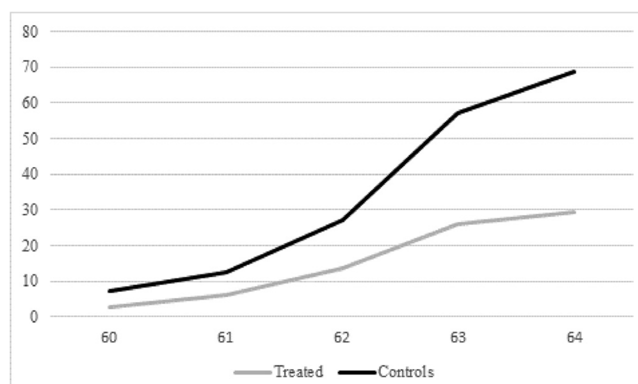

Fig. 9. Cumulative retirement by age for the treated and controls. This figure excludes individuals born in the first quarter of the year.

Table 6
Retired in 2014: Results from a linear probability model.

Dependent variable: Retired in 2014	(1)
Treatment dummy	−0.263***
	(0.023)
Birth date/100	−0.766***
	(0.069)
Birth date/100 * Treatment dummy	0.393***
	(0.101)
Constant	0.628***
	(0.017)
Observations	6476
Adjusted R^2	0.241

Robust standard errors are in parentheses.
*$p < 0.10$.
**$p < 0.05$.
*** $p < 0.01$.

distribution for the two cohorts, omitting those born in the first quarter of the year. At each age, the retirement rates of the controls are much higher, but also note that the retirement rates of the treated seem to lag by about one year (at least up until age 63). We therefore also estimate a Tobit model on the retirement age with right censoring at age 64. The results of this regression are reported in Table 7. The treatment coefficient implies an increase in the retirement age of 1.2 years (about 14 months).[21] In earlier sections, we also saw that about 16% of the affected cohort chose to participate in the LCS and that such workers intended to fully counter the effect of the reform. Unfortunately, those participating in the LCS were not considered retired when they took a self-financed leave before the actual date of retirement. We therefore cannot check this with the administrative data, but it is conceivable that a fraction of the workers considered in the data as retiring later could, in fact, have stopped working earlier. The estimate in Table 6 can therefore be

[21] Omitting individuals born in the first quarter of the year reduces this figure to 1.013 years (s.e. = 0.167).

Table 7
Tobit model on retirement age (right censored at age 64).

Dependent variable:	(1) Retirement age (right censored)
Treatment dummy	1.207***
	(0.109)
Birth date/100	0.283
	(0.346)
Birth date/100 * Treatment dummy	0.902*
	(0.520)
Constant	62.990***
	(0.075)
Observations	6702

Robust standard errors are in parentheses.
** $p < 0.05$.
* $p < 0.10$.
*** $p < 0.01$.

viewed as an upper bound of the true effect. We estimate a model where we assume that all LCS participants were retired by March 2014, which reduces the coefficient to −0.218 (s.e. = 0.024). We can thus set the true effect of being retired in 2014 as bound within the interval [−0.263, −0.218]. Unfortunately, it is not possible to determine the bounds of the exact age of retirement (see Table 7) unless one is willing to make strong assumptions.

4.4. Additional analyses and sensitivity

Figs. 1a–1c show that PPW accrual rates are higher in the new system, but also that the PPW of the new and old systems converge at or just after the statutory retirement age. We therefore examine whether there is a differential response to the reform between workers who expected in 2002 to retire prior to age 60 and those who expected to retire at or after the statutory retirement age (65). Table C5 in Appendix C shows that those who initially expected to retire at or after age 65 do not increase their private savings.

We also examine whether there is a differential treatment effect across wage, education, and health. We did not find significantly different treatment effects for expectations and savings. For retirement realizations, we find that higher-educated workers are less likely to be retired in 2014 than lower-wage workers.

The RDD regressions of the previous section could be subject to threats concerning the specification and sample selection that could influence our results. First, we re-estimate all the models using the optimal bandwidth approach of Imbens and Kalyanaraman (2012). Table C6 in Appendix C shows that the results across all outcome variables are hardly affected. Second, although graphical evidence indicates no age effects, we also estimate RDD models with quadratic age effects.[22] In all but one case, these regressions do not affect the outcome (the results are shown in Appendix D).

Third, the reform implied that the treated could not retire before the age of 60, whereas the controls still could. These changes in the left tail of the retirement distribution could influence the treatment effect, as well as its interpretation. To verify this, we also re-estimate the models for all the outcome variables, excluding individuals who retired before age 60. Note that this stratification invalidates a causal interpretation of the results. The results are reported in Table C7 in Appendix C and show that this did not significantly alter the effects across all outcome variables.

Fourth, the sample attrition is substantial. In Appendix B, we examine the survey participation patterns for the treated and control groups and show that attrition does not depend on the treatment status. It is, however, still conceivable that especially for the longer run effects changes in the treatment effects might be due to changes in the composition of the sample. To further look into this we re-estimated the models for longer-run expectations outcomes regressions, using the sample of individuals who were present in all waves (see Table C3 in Appendix C). While this substantially reduces the number of observations, this only marginally changed the treatment effects.

Finally, the pension fund ABP covers the central government, local government, and privatized (formerly government) sectors, the latter being primarily utility sectors (energy and electricity) and railway transportation. The behavior of workers in these sectors could be different from that of other public sector workers. Indeed, they are slightly overrepresented in the group of treated participants in the LCS. In Table C8 in Appendix C, we show the results for these workers. The table shows that the treatment effect of participation in the LCS is indeed much larger than the effects of about eight percentage points in our main analyses. However, with respect to the other outcome variables, we find only marginal changes.

5. Summary, discussion, and conclusion

This paper looks at the response of retirement expectations, actual retirement, and savings to a pension reform. The reform affected workers born in 1950 (or later) and led to a substantial loss in pension wealth. Those born prior to 1950 (controls) were not affected and could retire at the age of 62 with a replacement rate of 70% of the average of wages earned in the past 10 years. The affected cohort in our sample (treated) had to work 13 months longer to obtain the same replacement rate or they could retire at age 62 with a replacement rate of 64%. At the same time, the government launched a tax-facilitated savings program, the LCS, that permitted tax-free savings of up to 12% of one's annual earnings. This program was open to all workers (treated and controls).

We find strong behavioral responses to the reform. The expectation data show that, one year after the reform, affected workers expected, on average, to work about 10.5 months longer. The drop in pension wealth is equivalent to a retirement that is 13 months earlier, so our results suggest that the average treated worker makes up for the drop in their pension wealth mainly at the expense of leisure in retirement and, to a lesser extent, by decreasing post-retirement consumption. The difference in retirement expectations between the treated and control groups remained stable in later years and is close to the differences in actual retirement rates observed in 2014. So, immediately after the reform the workers revised their plans and stuck to these in later years. It thus appears that that the reform was very effective in raising the retirement age for the larger part of the sample. Our results on retirement are consistent with earlier studies documenting important effects of changes in the incentive structure of Social Security and pensions on labor supply and earnings (Krueger and Pischke, 1992; Borsch-Supan, 2000; Coile and Gruber, 2007; Mastrobuoni, 2009; Blau and Goodstein, 2010; Gelber, Isen and Song, 2016; Fetter and Lockwood, 2018)

Participation in the tax-facilitated LCS was around 16% for the treated, which is more than twice the savings rate of the controls. The savings response remained stable in later years. Those in the control and treated cohorts who were participating in the program stated that they expected to finance an earlier retirement by about eight months and 13 months, respectively. The controls' savings of about eight months could be interpreted as a response to the large savings subsidy on LCS savings. These are additional savings. This

[22] Gelman and Imbens (2018) suggest using second-order polynomials, and not higher-order polynomials in RD designs.

result contrasts with those of Gale and Scholtz (1994), who find no effect on total savings when the contribution limits of tax-facilitated IRAs are increased.

For the treated, the savings effect is the sum of increased private savings induced by the drop in PPW and additional savings induced by the savings subsidy. Under some additional assumptions we can interpret the five-month difference between the treated and controls as a savings effect induced by the drop in pension wealth, that is, a pure crowding-in effect of 38% between public and private wealth. This result is similar to those of Bottazzi et al. (2006), who find a 30% substitution rate between public and private wealth, and in the range of estimates found in other studies (e.g., Feldstein and Pellechio, 1979; Gale, 1998; Bernheim, 2002; Engelgardt and Kumar, 2011; Chetty et al., 2014; Alessie et al., 2013).

The 16% of workers born in 1950 who participated in the LCS stuck to their previously planned retirement dates. These workers were mostly high-wage workers. For these workers, private wealth fully crowded out PPW. Those not participating in the LCS expected to postpone retirement by 12.5 months. These were generally lower-wage workers who, on average, were also in worse health. This result suggests that the zero substitution rate between private and public health is primarily due to financial constraints, rather than a low disutility of work.

The effects found here could have to do with the reform's implementation. The reform was unexpected, implemented shortly after its announcement, and late in the game for the workers studied here. At the time of the implementation, in January 2006, the affected cohort in our sample was 56 years old, leaving them with six years to save a one-year leave if they desired retiring at age 62 rather than at age 63. It is conceivable that this was not possible for the larger part of the workers. However, we cannot exclude the possibility that our findings also hold for younger low-wage workers, despite having a longer period to compensate for the loss in public pension wealth. Unfortunately, we cannot check this with the data at hand. We therefore leave this issue for future research.

The question is whether the strong labor supply effects of the pension reform on low-wage workers, who are often also in worse health, is desirable from the perspective of a government. A further unintended side effect of the introduction of the tax-facilitated savings plan is that, in particular, high-wage earners who participated in the savings program but who were not affected by the pension reform decided to retire even sooner than initially planned.

Do our results for older workers in the public sector have external validity? The public sector is atypical, in the sense that workers are, on average, much more educated and face different working conditions than workers in other sectors of the economy. In sectors with strenuous working conditions, such as construction, workers' health can limit the ability to extend their working life. Therefore, for such workers, the effect of a similar reform on actual retirement could be smaller. Workers in these sectors are also less educated, have lower wages, and are likely to have less wealth to compensate for losses in PPW. Unfortunately, our data do not allow us to examine this issue in more detail for workers in these sectors. We do, however, find similar treatment effects for workers in privatized (formerly public) sectors.

Although our findings are not generalizable to all workers, old and young, they do, however, point to important lessons for other countries that have implemented or are planning to implement pension reforms. Demographic changes and retirement patterns in the past require timely action. This means that reforms will involve a substantial group of older workers who are facing retirement in the short run. Reforms announced late in the game have substantial effects on retirement patterns, but most of the effect is confined to workers who are financially constrained and who cannot counter PPW losses with private wealth. These are generally lower-wage workers with lower education levels and generally worse health. The results are relevant to workers in all countries, irrespective of the specifics of the country's pension system.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A–E. Supplementary material

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jpubeco.2020.104297.

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